

# Response to arXiv:1005.2615

J.I. Collar<sup>a</sup> and D.N. McKinsey<sup>b</sup>

<sup>a</sup>*Enrico Fermi Institute, KICP and Department of Physics,  
University of Chicago, Chicago, IL 60637*

*and*  
<sup>b</sup>*Department of Physics,  
Yale University, New Haven, CT 06520*

The XENON100 collaboration has offered a Reply [1] to our Comments [2] on their first results [3]. We find it inadequate, for more than one reason. First, we deem that clear efforts are made in [1] to distort what are otherwise several straightforward points made by us in [2]. We believe that these cannot be missed by an attentive reader. In order to keep the discussion brief, rather than disputing these points one by one, we concentrate below on a number of broader related issues. Second, we sense an avoidance of the main criticism contained in our Comments. We start by restating this criticism below, and conclude this brief note with a challenge to the XENON100 collaboration.

1. It is simply not legitimate to base a dark matter cross-section limit on presumed Poisson fluctuations in S1 in an energy range where there are no measurements of  $\mathcal{L}_{\text{eff}}$  (i.e. below 4 keV<sub>r</sub>). If we accept as a community that it is not proper to take advantage of the Poisson fluctuations from a completely unquantified light source, the controversy about the behavior of  $\mathcal{L}_{\text{eff}}$  at low energies will be less relevant. In any case, we recall that  $\mathcal{L}_{\text{eff}}$  is the ratio between the (poorly understood) nuclear recoil scintillation yield and the (well understood and finite) electron recoil scintillation yield from 122 keV gamma rays. In the limit of low energy,  $\mathcal{L}_{\text{eff}}$  has to go to zero, because eventually there is not enough energy for the nuclear recoil to excite the xenon atom to its first excited state. The numerator in  $\mathcal{L}_{\text{eff}}$  must go to zero, and the denominator is finite. The question is where, not if,  $\mathcal{L}_{\text{eff}}$  goes to zero. While the XENON100 collaboration agrees with us that more accurate measurements of  $\mathcal{L}_{\text{eff}}$  are needed, when setting a limit on a dark matter cross-section it is most responsible to be conservative about the assumptions going into the sensitivity.
2. Note that to obtain our limit curves, in an attempt to replicate the XENON100 claimed limits, we did in fact include S1 fluctuations, and we succeed in reproducing their limits under the same set of questionable assumptions. Section 4 in [1] is rife with misleading references to what we trust is rather clearly stated in the main text and Appendix of our [2]. We encourage other researchers to attempt to replicate the XENON100 limit curve at low WIMP masses and to assess the effect of uncertainties in  $\mathcal{L}_{\text{eff}}$ . We firmly maintain that Fig. 5 in our Comments is representative of the degree of uncertainty

that can be presently assigned to light-WIMP studies using LXe as a target.

3. The Reply by XENON100 [1] calls attention to a systematic correction applied in Manzur *et al.* [4], based on trigger threshold. The work by Manzur *et al.* includes measurements in both single phase (scintillation only) and two-phase (scintillation plus charge) modes. Operating in two phase mode, the trigger was based on proportional scintillation from charge, allowing a trigger threshold well below the analysis threshold. Manzur *et al.* operated in both modes in order to cross-check the  $\mathcal{L}_{\text{eff}}$  results and make sure that they were robust. Values measured for  $\mathcal{L}_{\text{eff}}$  in both data acquisition modes were consistent.
4. In contrast to this, the recent  $\mathcal{L}_{\text{eff}}$  measurements described in Aprile *et al.* [5] used a trigger based only on scintillation light. Detail is lacking on the Monte Carlo used to generate the trigger efficiency correction and the uncertainty in the inputs to the simulation, which would contribute to the uncertainty in  $\mathcal{L}_{\text{eff}}$ . Uncertainties apparently not taken into account in that measurement include uncertainty in nuclear recoil energy resolution, and systematic uncertainty in subtracting the large multiple scattering background, which is dominated by events with small S1 signal. We note that because of the bulkiness of the liquid xenon cell used in [5] and the significant amount of inactive liquid xenon and PTFE surrounding the active liquid xenon volume, multiple scattering background is much more significant in the Columbia measurement [5] than in the Yale measurement [4]. Overall, a number of contributions to systematic error are ignored in the Aprile *et al.* [5] analysis, and we feel that that the claimed systematic errors are clearly underestimated.
5. For nuclear recoils in LXe, the charge yield per keV is found to increase as energy decreases. This very likely takes away signal that otherwise could have gone toward production of scintillation light. It is wishful thinking to presume that the scintillation signal remains constant while the charge yield increases. See the empirical model described in Manzur *et al.* [4].
6. We called attention to the two-body kinematic cut-off (generating an  $E_{\text{max}}$  in LXe of 39 keV) not to claim that this is the only effect relevant to liquid

xenon scintillation, but merely to point out that below this energy, it starts to become kinematically unfavorable for nuclear recoils to ionize xenon, which normally is the main process that leads to scintillation. In models of scintillator response, the scintillation yield does not drop immediately to zero, but begins to adiabatically decrease at this point. This is mentioned several times in our Comments, and is an effect consistent with data from other scintillators as well [2]. For LXe, in order for  $\mathcal{L}_{\text{eff}}$  to remain constant below 39 keV<sub>r</sub>, new unknown physical processes would have to be invoked to balance this effect and the effect of charge straggling described in [4]. Section 3 in [1] reads as an protracted discussion constructed to argue against fundamental concepts, widely regarded as common to all scintillators. We also notice a preoccupation in [1] to keep comparing the XENON100 claimed sensitivity with the presently favored DAMA region: as we have stated in [2], these same fundamental concepts can amplify the existing uncertainty in the NaI[Tl] quenching factors, affecting the position of the DAMA region in WIMP phase space, and in some plausible scenarios displacing it away from XENON100 constraints.

7. Referring to the discussion around Sorensen *et al.* [6], the first author on that paper has significantly improved the analysis of the XENON10 nuclear recoil calibration data [7]. The new analysis shows a  $\mathcal{L}_{\text{eff}}$  that decreases at lower energies. According to Sorensen, the lowest energy points (denoted by open circles in [7]) are not a claim of rising  $\mathcal{L}_{\text{eff}}$  as stated by XENON100, but instead an illustration of how  $\mathcal{L}_{\text{eff}}$  can be mistakenly found to rise if threshold effects are not properly taken into account.

At this point we believe that a reader inclined to follow the fine details of this discussion has been provided with enough information to develop an opinion on the

uncertainties affecting XENON100 results. However, one important argument remains to be made: it seems discernible to the trained eye that the acceptance showed in Fig. 3 in [3] cannot explain on its own the rapidly decreasing sensitivity to AmBe neutron recoils noticeable below  $\sim 6$  keV<sub>r</sub> in Fig. 2 in [3], when taking into account that the expected trend in such a calibration is a rapid rise in counting rate at low recoil energy (see for instance Fig. 2 in [2], this is an expected behavior common to fast neutron irradiations of targets comprising heavy nuclei). This can be interpreted as an indication of a decreasing  $\mathcal{L}_{\text{eff}}$  with decreasing energy in XENON100, similar to that recently found for XENON10 [7].

The present reluctance by the XENON100 collaboration to release a comparison between expected and measured response to neutron-induced nuclear recoils in their apparatus makes it very hard to quantify such impressions. Their attitude is isolated: it is customary in experimental searches for WIMPs (e.g. CDMS [8], COUPP [9], ZEPLIN [10], XENON10 [11], etc.) to calibrate the detectors with fast neutrons, limiting the energy region usable for the search to that for which an understanding of the response exists, or alternatively folding into the analysis a sensitivity penalty from any existing disagreements. These can have a fundamental origin as in the case of  $\mathcal{L}_{\text{eff}}$ , or an instrumental explanation as in the case of the recoil signal acceptance. Hence the importance of performing these calibrations *in situ*, using the same device dedicated to the WIMP search. We invite the XENON100 collaboration to revert to the mainstream by adopting these conservative practices. After extensive consultations, we believe to be speaking for the majority of the dark matter experimental community when making this request. This will result in a recovered credibility for present and future XENON100 claims. The alternative would involve a sort of magical thinking: to expect sensitivity to WIMP-induced nuclear recoils, when the response to neutron recoils of the same energy is weak or absent.

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